OBITUARY NOTICES OF FELLOWS DECEASED.

Richard Owen was born at Lancaster on July 20, 1804. His father, whose name was also Richard, was engaged in business connected with the West Indies. His mother's name was Catherine Parrin. He was educated at the Grammar School at Lancaster (where one of his schoolfellows was W. Whewell, afterwards Master of Trinity), apprenticed to a surgeon of the name of Harrison in that town, and studied surgery at the County Hospital. No evidence can now be found for the statement which has appeared in many biographical notices that when a boy he went to sea as a midshipman, nor is there any that at a later period he had an intention to enter the medical service of the Navy, or applied for and obtained an appointment, as has also been stated.

In 1824 he matriculated at the University of Edinburgh, and had the good fortune to attend the anatomical course of Dr. Barclay, then approaching the close of a successful career as an extra-academical lecturer, whose teaching was of a very superior order to that of the third Monro, who, by virtue of hereditary influences, happened at that time to be the University Professor of Anatomy. In his work 'On the Nature of Limbs,' Owen refers to "the extensive knowledge of comparative anatomy possessed by my revered preceptor in anatomy, Dr. Barclay," and always spoke of him with affectionate regard.

He did not remain in Edinburgh to take his degree, but removed to St. Bartholomew's Hospital in London, and passed the examination for the membership of the Royal College of Surgeons on August 18, 1826.

His first published scientific works were in the direction of surgical pathology, being on encysted calculus of the urinary bladder and on the effects of ligature of the internal iliac artery for the cure of aneurism.

At St. Bartholomew's Hospital he soon attracted the attention of the celebrated Abernethy, through whose influence he obtained the appointment of Assistant Conservator to the Hunterian Museum of the Royal College of Surgeons. This was in 1827, and it caused him to abandon the prospect of private practice, to which he had begun to devote himself while living in Serle Street, Lincoln's Inn Fields, for the more congenial pursuit of comparative anatomy. The Conservator of

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the Museum at that time was William Clift, John Hunter's last and most devoted pupil and assistant, under whose faithful guardianship the collection had been most carefully preserved during the long interval between the death of its founder and its transference to the custody of the College of Surgeons. From him, Owen early imbibed an enthusiastic reverence for his great master, which was continually augmented with the closer study of his collection and works, which now became the principal duty of his life. In 1830 and 1831 he visited Paris, where he attended the lectures of Cuvier and Geoffroy St. Hilaire, and worked in the dissecting rooms and public galleries of the Jardin des Plantes. In 1835 he married Clift's only daughter, Caroline, and in 1842 was associated with him as joint Conservator of the Museum. On Clift's retirement soon after, he became sole Conservator, with Mr. J. T. Quekett as Assistant.

He was appointed Hunterian Professor of Comparative Anatomy and Physiology in 1835, an office which he held until his retirement from the College in 1856, and from which he took the title of "Professor Owen," by which he was far more widely known than by the knightly addition of his later years.

Until the year 1852, when the Queen gave him the charming cottage called Sheen Lodge in Richmond Park, where he resided to the end of his life, he occupied small apartments within the building of the College of Surgeons; these, however inconvenient they might be in some respects, furnished him with unusual facilities for pursuing his work by night as well as day in the museum, dissecting rooms, and library, of that institution.

Owen's life of scientific activity may be divided into two periods, during each of which the nature of his work was determined to a considerable extent by the circumstances by which he was environed. Each of these periods embraces a term of very nearly thirty years. The first, from 1827 to 1856, was spent at the Royal College of Surgeons; the second, from 1856 to 1884, in the British Museum. It was in the first that he mainly made his great reputation as an anatomist, having utilised to the fullest possible extent the opportunities which were placed in his way by the care of the Hunterian Museum. For many years he worked in that institution under the happiest of auspices. From the routine and drudgery which always take up so large a portion of the time of a conscientious museum curator, he was relieved by the painstaking, methodical, William Clift; the far more gifted son-in-law being thus able to throw himself to his heart's content into the higher work of the office. This at first mainly consisted in the preparation of that monumental 'Descriptive and Illustrated Catalogue of the Physiological Series of Comparative Anatomy,' founded upon Hunter's preparations, largely added to by Owen himself, which was published in five quarto volumes between the

years 1833 and 1840. This work, which has been taken as a model for many other subsequently published catalogues, contains a minute description of nearly four thousand preparations. The labour involved in preparing it was greatly increased by the circumstance that the origin of a large number of them had not been preserved, and even the species of the animals from which they were derived had to be discovered by tedious researches among old documents, or by comparison with fresh dissections. It was mainly to aid him in this work that he engaged upon the long series of dissections of animals which died from time to time in the Gardens of the Zoological Society, the descriptions of which, as published in the Proceedings and Transactions of the Society, form a precious fund of information upon the comparative anatomy of the higher Vertebrates. The series commences with an account of the anatomy of an Orang Utan, which was communicated to the first scientific meeting of the Society, held on the evening of Tuesday, November 9, 1830, and was continued with descriptions of dissections of the Beaver, Suricate, Acouchy, Thibet Bear, Gannet, Crocodile, Armadillo, Seal, Kangaroo, Tapir, Toucan, Flamingo, Hyrax, Hornbill, Cheetah, Capybara, Pelican, Kinkajou, Wombat, Giraffe, Dugong, Apteryx, Wart-hog, Walrus, Great Ant-eater, and many others.

Among the many obscure subjects in anatomy and physiology on which he threw much light by his researches at this period were several connected with the generation, development, and structure of the Marsupialia and Monotrema, groups which always had great interest for him. It is a curious coincidence that his first paper communicated to the Royal Society (in 1832) "On the Mammary Glands of the *Ornithorhynchus paradoxus*" was one of a series which only terminated in almost the last which he offered to the same Society (in 1887), being a description of a newly excluded young of the same animal, published in the 'Proceedings,' vol. 42, p. 391.

On the completion of the 'Catalogue of the Physiological Series' his curatorial duties led him to undertake the catalogues of the osteological collections of recent and extinct forms. This task necessitated minute studies of the modifications of the skeleton in all vertebrated animals, and researches into their dentition, the latter being finally embodied in his great work on 'Odontography' (1840–45), in which he brought a vast amount of light out of what was previously chaotic in our knowledge of the subject, and cleared the way for all future work upon it. Although recent advances of knowledge have shown that there are difficulties in accepting the whole of Owen's system of homologies and notation of the teeth of Mammals, it was an immense improvement upon anything of the kind which existed before, and a considerable part of it seems likely to remain a permanent addition to our means of describing these

organs. The close study of the bones and teeth of existing animals was of extreme importance to him in his long continued and laborious researches into fossil forms; and, following in the footsteps of Cuvier, he fully appreciated and deeply profited by the dependence of the study of the living in elucidating the dead, and *vice versâ*. Perhaps the best example of this is to be seen in his elaborate memoir on the Mylodon, published in 1842, entitled 'Description of the Skeleton of an Extinct Gigantic Sloth (*Mylodon robustus*, Owen), with Observations on the Osteology, Natural Affinities, and Probable Habits of the Megatheroid Quadrupeds in General,' a masterpiece both of anatomical description and of reasoning and inference. A comparatively popular outcome of some of his work in this direction was the volume on 'British Fossil Mammals and Birds,' published in 1844–46, as a companion to the works of Yarrell, Bell, and others on the recent fauna of our island. He also wrote, assisted by Dr. S. P. Woodward, the article "Palæontology" for the 'Encyclopædia Britannica,' which, when afterwards published in a separate form, reached a second edition in 1861.

To this first period of his life belong the courses of Hunterian Lectures, given annually at the College of Surgeons, each year on a fresh subject, and each year the means of bringing before the world new and original discoveries which attracted, even fascinated, large audiences, and did much to foster an interest in the science among cultivated people of various classes and professions. They also added greatly to the scientific renown of the College in which they were given. To this period also belong the development and popularisation of those transcendental views of anatomy—the conception of creation according to types, and the construction of the Vertebrate archetype—views which had great attractions and even uses in their day, and which were accepted by many, at all events as working hypotheses around which facts could be marshalled, and out of which grew a methodical system of anatomical terminology, much of which has survived to the present time. The recognition of homology and its distinction from analogy, which was so strongly insisted on by Owen, marked a distinct advance in philosophical anatomy. These generalisations, first announced in lectures at the College of Surgeons, were afterwards embodied in two works: 'The Archetype and Homologies of the Vertebrate Skeleton' (1848) and 'The Nature of Limbs' (1849).

The contributions which Owen made to our knowledge of the structure of Invertebrate animals nearly all belong to the earlier period of his career, one of the most important being his admirable and exhaustive memoir on the Pearly Nautilus founded on the dissection of a specimen of this, at that time exceedingly rare, animal, sent to him in spirit by his friend Dr. George Bennett, of Sydney. This

was illustrated by carefully executed drawings by his own hand, and published in the year 1832, when he was only 27 years of age. The Cephalopoda continued to engage his attention, and the merits of a memoir on fossil Belemnites from the Oxford Clay, published in the 'Philosophical Transactions' in 1844, was the cause assigned for the award to him of the Royal Medal in 1846. He contributed the article "Cephalopoda," to the 'Cyclopædia of Anatomy and Physiology' (1836), catalogued the extinct Cephalopoda in the Museum of the Royal College of Surgeons (1856), and wrote original papers on *Clavagelia* (1834), *Trichina spiralis* (1835), *Linguatula* (1835), *Distoma* (1835), *Spondylus* (1838), *Euplectella* (1841), *Terebratula* (in the introduction to Davidson's classical 'Monograph of the British Fossil Brachiopods,' 1853), and many other subjects, including the well-known essay on "Parthenogenesis, or the Successive Production of Procreating Individuals from a Single Ovum" (1849).

In 1843 his 'Lectures on the Comparative Anatomy and Physiology of the Invertebrate Animals,' in the form of notes taken by his pupil Mr. W. White Cooper, appeared as a separate work. Of this, a second expanded and revised edition was published in 1855. By this time, as the Royal Society's 'Catalogue of Scientific Papers' shows, he had been the author of as many as 250 separate scientific memoirs.

In 1856, when Owen had reached the zenith of his fame, and was recognised throughout Europe as the first anatomist of the day, a change came over his career. Difficulties with the governing body of the College of Surgeons, arising from his impatience at being required to perform what he considered the lower administrative duties of his office, caused him readily to take advantage of an offer from the Trustees of the British Museum to undertake a newly created post, that of Superintendent of the Natural History Departments of the Museum. It was thought that hitherto these departments, being under the direct control of a chief who had been invariably chosen from the literary side of the establishment, and whose title in fact was that of "Principal Librarian," had not obtained their due share of attention in the general and financial administration, and that if they were grouped together an placed under a strong administrator, who should be able to exercise influence in advocating their claims to consideration, and who should be responsible for their internal working, their relative position in the establishment would be improved. Owen was accordingly placed in this position, with a salary of £800 a year,* and bade farewell to the College of Surgeons, its museum, and its lectures. At the British

* It may be mentioned that he was already in receipt of a Civil List pension of £200 a year, accorded to him in consideration of his scientific work, mainly in the completion of the catalogue of the Hunterian Collections.

Museum, however, he encountered the difficulties which are nearly always experienced by an outsider suddenly imported into the midst of an existing establishment without any very well-defined position. The Principal Librarian, Panizzi, was a man of strong will and despotic character, and little disposed to share any of his authority with another. The heads of the departments, especially Dr. J. E. Gray, Keeper of Zoology, preferred to maintain the independence to which they were accustomed within their own sphere of action, and to have no intermediary between them and the Trustees, except the Principal Librarian, who though perhaps with little sympathy, had also, from lack of special knowledge, but little power of interference in detail. Hence Owen found himself in a situation the duties of which were little more than nominal, probably for him the best that could have been, as it gave his indomitable industry full play in the directions for which his talents were best fitted, and with the magnificent material in the collections of the Museum at his command, he set to work with great vigour upon a renewed series of researches, the results of which for many years taxed the resources of most of the scientific societies of London to publish. It followed from the nature of the materials that came most readily to his hand, and the smaller facilities for dissection now available than those afforded by the College of Surgeons, that his original work was henceforth mainly confined to osteology, and chiefly to that of extinct animals. The rich treasures of the palæontological department were explored, named and described, as were also the valuable additions which poured in from various parts of the world, attracted in many cases by Owen's great reputation. The long series of papers on the gigantic extinct Birds of New Zealand, begun in the year 1839, at the College of Surgeons, with the receipt of the fragment of a femur, upon which the first evidence of their existence was based, was now continued at intervals as fresh materials arrived. The Marsupials of Australia, the Edentates of South America, the Triassic Reptiles from South Africa, the *Archæopteryx* from Solenhofen, the Mesozoic Mammals from the Purbeck, the Aborigines of the Andaman Islands, the Cave remains, human and otherwise, of the South of France, the Cetacea of the Suffolk Crag, the Gorilla and other Anthropoid Apes, the Dodo, Great Auk, and *Chiromys*, and many other remarkable forms of animal life were all subjects of elaborate memoirs from his untiring pen. These were adorned in every case with a profusion of admirable illustrations, drawn as often as possible of the full size of nature. His contributions to the publications of the Palæontographical Society, mainly upon the extinct Reptiles of the British Isles, fill more than a thousand pages, and are illustrated by nearly three hundred plates.

He now also found leisure to perform the pious duty of vindicat-

ing the scientific reputation of his great predecessor, John Hunter, by arranging and revising for publication a large collection of precious manuscripts containing records of dissections of animals and observations and reflections upon numerous subjects connected with anatomy, physiology, and natural history in general. These were published in 1861, in two closely printed octavo volumes, entitled 'Essays and Observations in Natural History, Anatomy, Physiology, Psychology, and Geology, by John Hunter, being his Posthumous Papers on those subjects.' The original manuscripts had been destroyed by Sir Everard Home, in 1823, but fortunately not before William Clift had taken copies of the greater part of them, and it was from these copies that the work was compiled. Its publication shows that Hunter, while occupied with a large and anxious practice—in itself labour enough for an ordinary man—while cultivating with a passionate energy the sciences of physiology and pathology, while collecting and arranging a museum such as has never been formed before or since by a single individual, had also carefully recorded a series of dissections of different species of animals which, as his editor justly says, "if published *seriatim*, would not only have vied with the labours of Daubenton, as recorded in the 'Histoire Naturelle,' of Buffon, or with the 'Comparative Dissections' of Vicq d'Azyr, which are inserted in the early volumes of the 'Encyclopédie Méthodique' and the 'Mémoires de l'Académie Royale de France,' but would have exceeded them both together."

In 1866 were published the first and second volumes, and in 1868 the third volume, of Owen's own great book on the Anatomy and Physiology of the Vertebrates.

This is the most encyclopædic work on the subject accomplished by any one individual since Cuvier's 'Leçons d'Anatomie Comparée,' and contains an immense mass of information mainly based upon original observations and dissections. It is in fact a collection of nearly all his previous memoirs arranged in systematic order, generally in the very words in which they were originally written, and unfortunately sometimes without the revision which advances made in the subject by the labours of others would have rendered desirable. Very little of the classification adopted in this work, either the primary division of the Vertebrates into Hæmatocrya and Hæmatotherma, or the divisions into classes and sub-classes, has been accepted by other zoologists. The division of the Mammalia into four sub-classes of equivalent value, upheld by Owen, not only in this work, but in various other publications issued about the same time (Rede Lecture, &c.), founded upon cerebral characteristics, was especially open to criticism. Though the separation of the Monotremes and Marsupials from all the others as a distinct group

(Lyencephala) is capable of vindication, the three other sub-classes, Lissencephala, Gyrencephala, and Archencephala, grade so imperceptibly into each other that their distinction as sub-classes cannot be maintained. The proposed definition of the distinguishing characters of the brain of Man (Archencephala) from that of other Mammals gave rise to a somewhat acute controversy, the echoes of which reached beyond the realms of purely scientific literature. On the other hand, the radical distinction between the two groups of Ungulates, the odd-toed and the even-toed, first indicated by Cuvier, when treating of the fossil forms, was thoroughly worked out by Owen through every portion of their organisation, and remains as a solid contribution to a rational system of classification.

The chapter called "General Conclusions" at the end of the third volume is devoted to a summary of his views on the principal controverted biological questions of the day, especially in relation to the teaching of Darwin, just then coming into great prominence. Although from the peculiarly involved style of Owen's writing, especially upon these subjects, it is sometimes difficult to define his real opinions, it appears that before the publication of the 'Origin of Species,' he had "been led to recognise species as exemplifying the continuous operation of natural law, or secondary cause, and that not only successively but progressively." Darwin's special doctrine of "natural selection," however, he never appreciated, and his strong opposition to it caused him, though quite erroneously, to be looked upon by those outside the world of science as a supporter of the old-fashioned and then more "orthodox" view of special creation. His most distinct utterance upon this subject is contained in the following paragraph:—"So, being unable to accept the volitional hypothesis, or that of impulse from within, or the selective force exerted by outward circumstances, I deem an innate tendency to deviate from parental type, operating through periods of adequate duration, to be the most probable nature, or way of operation, of the secondary law, whereby species have been derived one from the other."—(*Op. cit.*, vol. 3, p. 807.)

His career as a lecturer did not entirely cease with his connexion with the College of Surgeons, as, by permission of the authorities of the Museum of Practical Geology in Jermyn Street, he gave several courses on the fossil remains of animals, open to the public, in the theatre of that institution, and he held in the years 1859, 1860, and 1861, in conjunction with his office at the British Museum, the Fullerian Professorship of Physiology in the Royal Institution. On the revival of the annual lecture on Sir Robert Rede's foundation in the University of Cambridge, in 1859, he was appointed to give the first, and took for his subject the Classification of the Mammalia. He also occasionally lectured at the Royal Institution on Friday

evenings, his last appearance there being on April 26, 1861, when he delivered the discourse "On the Scope and Appliances of a National Museum of Natural History," to be presently referred to. In April, 1862, he gave four lectures on Birds at the London Institution.

While at the College of Surgeons he had been a member of a Government Commission for enquiring into the health of the Metropolis; and subsequently (in 1849) of one on Smithfield and the other meat markets, in which he strongly advocated the entire suppression of intramural slaughter-houses, and the concomitant evil of the passage of droves of sheep and cattle through the streets of London. For the Great Exhibition of 1851 he was on the Preliminary Committee of Organisation, and he acted as Chairman of the Jury on raw materials, alimentary substances, &c., and published an elaborate report on their awards. He also delivered to the Society of Arts a lecture on "Raw Animal Products, and their Uses in Manufacture." Similar services were performed by him for the Exposition Universelle of Paris in 1858.

It has been already said that Owen took scarcely any part in the details of the administration of the British Museum, but one subject relating to that establishment did largely engage his attention from his first connexion with it. That the accommodation afforded by the rooms devoted to natural history in the Museum at Bloomsbury was painfully inadequate for the purpose was evident to him as well as to everyone else. Space must be obtained somewhere, even for the proper conservation and display of the existing collections, to say nothing of the vast additions that must be expected if the subject were to be represented in anything like the way in which it deserved to be in his eyes, and Owen in this respect had very large views. The scientific public, the officers of the Museum, and the Trustees were much divided as to whether it would be better to endeavour to obtain this space in the neighbourhood of the existing Museum, or to remove a portion of the collection to a totally distinct locality. After some apparent hesitation, Owen threw himself strongly on the side of those who took the latter view, being the one which seemed to him to have the best chance of leading to a successful result, and he strongly urged upon the Government, and upon the public generally, in annual Museum returns, lectures, and pamphlets, the desirability of the scheme. In his address as President of the Biological Section of the British Association at the York meeting in 1881, he has given a history of the part he took in promoting the building of the new museum at South Kensington, including his success in enlisting the sympathy of Mr. Gladstone, by whose powerful aid the difficulties and opposition with which the plan was met in Parliament were mainly overcome. His earlier views upon the subject are fully

explained in a small work entitled 'On the Extent and Aims of a National Museum of Natural History,' published in 1862, being an expansion of the lecture he gave at the Royal Institution in the previous year. Much controversy arose about this time as to the best principle of museum organization, Owen adhering to the old view of a public exhibition on a very extensive scale, while the greater number of naturalists of the time preferred the system of dividing the collections into a comparatively limited public exhibition, the bulk of the specimens being kept in a manner accessible only to the researches of advanced students. The Royal Commission on the Advancement of Science, of which the Duke of Devonshire was Chairman, investigated the subject fully, and reported (in 1874) in favour of the latter view; but in the new building at South Kensington there was, unfortunately, little provision made for carrying it out in a satisfactory manner.

As long ago as 1859, in one of his reports on the subject to the Trustees, Owen recommended that the new museum building, "besides giving the requisite accommodation to the several classes of natural history objects, as they had been by authority exhibited and arranged for public instruction and gratification, should also include a hall or exhibition space for a distinct department, adapted to convey an elementary knowledge of the subjects of all the divisions of natural history to the large proportion of public visitors not specially conversant with any of those subjects." The same idea, in a later publication, is thus described:—"One of the most popular and instructive features in a public collection of natural history would be an apartment devoted to the specimens selected to show type characters of the principal groups of organised and crystallised forms. This would constitute an epitome of natural history, and should convey to the eye, in the easiest way, an elementary knowledge of the sciences." In every modification which the plans of the new building underwent, a hall for the purpose indicated in the above passages formed a prominent feature, being in the later stages of the development of the building, called, for want of a better name, the "Index Museum." Though Owen gave the suggestion and designed the general plan of the hall, the arrangement of its contents was left to his successor to carry out.

In another part of his original scheme he was less successful. The lecture theatre which he had throughout urged with great pertinacity as a necessary accompaniment to a natural history museum was, as he says in the address referred to above, "erased from my plan, and the elementary courses of lectures remain for future fulfilment."

On several other important questions of museum arrangement, Owen allowed his views, even when essentially philosophical as well

as practical, to be overruled. As long ago as December, 1841, he submitted to the Museum Committee of the Royal College of Surgeons the question of incorporating in one catalogue and system of arrangement the fossil bones of extinct animals with the specimens of recent osteology, and shortly afterwards laid before the Committee a report pointing out the advantages of such a plan. Strangely enough, though receiving the formal approval of the Council, no steps were taken to carry it out as long as he was at the College. He returned to the question in reference to the arrangement of the new National Museum, and although no longer advocating so complete an incorporation of the two series, apparently in consideration of the interests of the division into "departments" which he found in existence there, he says "The Department of Zoology in such a museum should be so located as to afford the easiest transit from the specimens of existing to those of extinct animals. The geologist specially devoted to the study of the evidence of extinct vegetation ought, in like manner, to have means of comparing his fossils with the collections of recent plants."* Provision for such an arrangement is clearly indicated in all the early plans for the building in which the space for the different subjects is allocated, but not a trace of it remained in the final disposition of the contents of the Museum, as Owen left it in 1883.

Another essential feature of Owen's original plan, without which, he says, "No collection of zoology can be regarded as complete," is a gallery of physical ethnology, the size of which he estimated (in 1862) at 150 ft. in length by 50 ft. in width. It was to contain casts of the entire body, coloured after life, of characteristic parts, as the head and face, skeletons of every variety arranged side by side for facility of comparison, the brain preserved in spirit, showing its characteristic size and distinctive structures, &c. "The series of zoology," he says, "would lack its most important feature were the illustrations of the physical characters of the human race to be omitted."

An adequate exhibition of the Cetacea, both by means of stuffed specimens and skeletons, also always formed a prominent element in his demand for space. "Birds, shells, minerals," he wrote, "are to be seen in any museum; but the largest, strangest, rarest specimens of the highest class of animals can only be studied in the galleries of a national one." And again: "If a national museum does not afford the naturalist the means of comparing the Cetacea, we never shall know anything about these most singular and anomalous animals."

When, however, the contents of the museum were finally arranged, nominally under his direction, physical anthropology was only repre-

* 'On the Extent and Aims of a National Museum of Natural History,' 2nd edit., 1862, p. 7.

sented by a few skeletons and skulls placed in a corner of the great gallery devoted to the osteology of the Mammalia, and the fine series of Cetacean skeletons could only be accommodated in a most unsuitable place for exhibition in a part of the basement not originally destined for any such purpose. The truth is that the division of the museum establishment into four distinct departments, each with its own head, left the "superintendent" practically powerless, and Owen's genius did not lie in the direction of such a reorganisation as might have been effected during the critical period of the removal of the collections from Bloomsbury and their installation in the new building. Advancing age, also, probably indisposed him to encounter the difficulties which inevitably arise from interference with time-honoured traditions. At length, at the close of the year 1883, being in his eightieth year, he asked to be relieved from the responsibilities of an office the duties of which he had practically ceased to perform.

The nine remaining years of his life were spent in peaceful retirement at Sheen Lodge, an ideal residence for one who had such a keen enjoyment of the charms of nature in every form, for, though so large a portion of his active life had been passed among dry bones, anatomical specimens, microscopes, and books, he retained a genuine love for out door natural history, and the sight of the deer and other animals in the park, the birds and insects in the garden, the trees, flowers, and varying aspects of the sky, filled him with enthusiastic admiration. He also had his library around him, and it is needless to say that the habit of strenuous work never deserted him till failing memory and bodily infirmity made it no longer possible to continue that flow of contributions to scientific literature which had never ceased during a period of sixty-two years, his first and last papers being dated respectively 1826 and 1888. His wife and only son had died some time before, but the son (who had held an appointment in the Foreign Office) left a widow and seven children, who, coming to reside with him at Sheen, completely relieved his latter days of the solitude in which they would otherwise have been passed. During the summer of 1892 his strength gradually failed, and he died on the 18th of December, literally of old age. In accordance with his own expressed desire, he was buried in the churchyard of Ham, near Richmond, in the same grave with his wife, a large and representative assemblage of men of science being present at the funeral ceremony.

It may be thought that the prodigious amount of work that Owen did in his special subjects would have left him no time for any other occupations or relaxations, but this was by no means the case. He was a great reader of poetry and romance, and could repeat by heart, even in his old age, page after page of Milton and other favourite authors, for he was gifted with a wonderful memory. For music he had a positive passion; in the most busy period of his life he might

constantly be seen at public concerts, listening with rapt attention, and in his earlier days was himself no mean vocalist, and acquired considerable proficiency in playing the violoncello. He was also a neat and careful draughtsman; the large number of anatomical sketches he left behind him testify to his industry in this direction. His handwriting was unusually clear and finished, considering the vast quantity of manuscript that flowed from his pen, for he rarely resorted to dictation or any labour-saving process. Only those who have had to clear out rooms, official or private, which have been long occupied by him can have any idea of the quantity of memoranda and extracts which he made with his own hand, and most of the books he was in the habit of using were filled with notes and comments.

Owen's was a very remarkable personality, both physically and mentally. He was tall and ungainly in figure, with massive head, lofty forehead, curiously round, prominent and expressive eyes, high cheek bones, large mouth and projecting chin, long, lank, dark hair, and during the greater part of his life, smooth-shaven face, and very florid complexion. Though in his general intercourse with others usually possessed of much of the ceremonial courtesy of the old school, and when in congenial society a delightful companion, owing to his unfailing flow of anecdote, considerable sense of humour, and strongly-developed faculty of imagination, he was not only an extremely adroit controversialist, but no man could say harder things of an adversary or rival. Unfortunately he was often engaged in controversy, a circumstance which led to a comparative isolation in his position among men who followed kindred pursuits, which was doubtless painful to himself as well as to others. It was this, combined with a certain inaptitude for ordinary business affairs, which was the cause of his never having been called to occupy several of the distinguished official positions in science to which his immense labours and brilliant talents would otherwise have fairly entitled him. Over the British Association he presided at the meeting at Leeds in 1858, and he had his full share of those honours and dignities to which a scientific man can aspire which involve no corresponding duties or responsibilities. He was made a C.B. in 1873, and a K.C.B. on his retirement from the Museum in 1884. He received the Prussian Order "Pour le Mérite" in 1851, the Cross of the French Legion of Honour in 1855, and was also decorated by the King of Italy with the Order of St. Maurice and St. Lazarus, and by the Emperor of Brazil with the Order of the Rose. He was chosen one of the eight foreign Associates of the Institute of France in 1859. The Universities of Oxford, Cambridge, and Dublin conferred upon him their honorary degrees, and he was an honorary or corresponding member of nearly every important scientific society in the world. The Geological Society presented him with the Wollaston Medal in 1838, and the

Royal College of Surgeons with its Honorary Gold Medal in 1883. He was the first to receive the gold medal established by the Linnean Society at the centenary meeting of that body in 1888. The Royal Society, of which he became a Fellow in December, 1834, and on the Council of which he served for five separate periods, awarded him one of the Royal Medals in 1846, and the Copley Medal in 1851.

W. H. F.

Sir William Aitken was born at Dundee on April 23, 1825, and received his early education in the High School of that town. He commenced the study of medicine under his father, a medical man in Dundee, and by attendance in the wards of the Dundee Royal Infirmary. In November, 1842, he matriculated in the University of Edinburgh, where, after attending lectures in the faculty of arts, and having complied with the requirements of the medical curriculum, he took the degree of Doctor of Medicine in 1848, his thesis on a pathological subject on that occasion gaining for him a gold medal. He also became a Licentiate of the Royal College of Surgeons of Edinburgh in the same year. Thence he appears to have proceeded to the University of Glasgow as Demonstrator of Anatomy under Dr. Allen Thomson. This office he continued to fill in conjunction with that of Pathologist to the Royal Infirmary of Glasgow up to 1855. Here he laid the foundation of that knowledge of disease which procured for him the appointment as Pathologist to the Hospitals of the Bosphorus, which were then filled by sufferers from the army in the Crimea. In association with the late Dr. Lyon he published a report on the diseases of the Crimea, which appeared in a Blue-book in 1856, and it is, and always will be, a valuable work of reference in regard to the maladies which were so fatal to the troops in that campaign.

On the foundation of the Army Medical School, which commenced its existence in 1860 at Chatham (afterwards transferred to Netley), and was an outcome of the experience of the Crimean War, Dr. Aitken was made Professor of Pathology, an appointment for which his early training and matured experience in the military hospitals in the East peculiarly fitted him, and which his subsequent career at Netley has abundantly justified. This duty he continued to perform until April, 1892, when failing health compelled him to rest from work. His final resignation of the chair had been fixed for the close of the session in July, 1892; but renal disease, from which he had for some time suffered, to the profound regret of his colleagues and numerous friends, terminated his valuable life on June 25, 1892.

Of the value of Aitken's work at the Army Medical School, as well as to medicine generally, it would be difficult to speak too highly. As a teacher he was pre-eminently successful in his method of imparting

knowledge; his reasoning was scientific and practical, his demonstrations lucid and convincing, and he must be gratefully remembered by hundreds of medical officers who owe much of their knowledge of disease, its causes and results, to his teaching.

A friend and colleague of Dr. Aitken writes:—"In the *post-mortem* room he was *facile princeps.* I never saw any one to compare with him at work of this kind. It was a lesson none could forget to see him conduct a *post-mortem* and hear his exposition of what he saw. He had great powers of work, and was a student in his own way all his life. His book held the field for many years as a student's text-book." And, again, "He was scrupulously honest as a writer; strove always to give every man his due."

Aitken's services to medicine were not restricted to his work as a teacher and examiner. He made many contributions of importance to the literature of medicine, and to that branch of it which he had made peculiarly his own—pathology. Up to the last he continued his labours, and at the time of his last illness was engaged in the publication of a descriptive catalogue of the Museum of Pathology now located at Netley. It is to be hoped that some competent successor will undertake to carry on and complete the work thus unfortunately interrupted.

It is sufficient to name the chief of his writings to indicate the debt due to this great pathologist, and to show how earnestly he laboured to contribute his share of knowledge to the common stock. The following are the best known:—

"On Inflammatory Effusions into the Substance of the Lungs as modified by Contagious Fevers," 1849. (2) "Contributions to Pathology." (3) "On the Pathology of the Diseases of the Troops in the East during the Russian War, 1855–56," in conjunction with Dr. R. D. Lyons. (4) "On the Diseases of the Troops in the East during the Russian War, and on the Climate of Scutari, on the Bosphorus," 1857. (5) "Medical History of War with Russia," 1857. (6) "On the Persistent and Pernicious Influence of the Residence in Bulgaria on the Subsequent Health of the British Troops in the Crimea." (7) "On conducting *Post-mortem* Examinations at Coroners' Inquests," 1857. (8, 9, 10) "On the Pathological Connexions and Relations of Epidemic Diseases in Man and the Lower Animals, with special reference to the relationship between the health of man and the condition of his food," 1857. (11) "Analytical Review of the Transactions of the Medico-Chirurgical Society of London, vol. xii," 1859. (12) "Critical and Analytical Review of Recent Works on the Pathology of Vaccination, and its Protective Influence from Small-pox," 1857. (13) "Analytical and Critical Review of the First Decennium of the Pathological Society of London," 1858. (14) "Handbook of the Science and Practice of

Medicine," 1858 [this has reached its seventh edition]. (15) "On the Growth of the Recruit and the Young Soldier" [now in its second edition]. (16) "On the Doctrine of Evolution in its Application to Pathology," 1885–86. (17) "On the Animal Alkaloids."

Aitken was a man of somewhat reserved and reticent speech, but what he said was pregnant with science and common-sense. He was of a most kindly, genial nature, loyal to his profession, devoted to his friends, and just to all. His personal character endeared him to every one. His frank, straightforward mode of expressing his opinions, tempered as they were by sound judgment and discretion, made him respected and esteemed, and contributed, in no small measure, to the formation of the reputation of one of that small but remarkable group of men to whom the great Army Medical School owes its rise, development, and success. Regretted universally by friends and colleagues, it is in the great School of Military Medicine, which owes him so much, that his loss will be most keenly felt.

His merits have not escaped some recognition. He was made a Fellow of the Royal Society in 1873. In 1887 he received the honour of knighthood. The Universities of Edinburgh and Glasgow, in 1888, conferred on him the degree of LL.D., whilst on the walls of the ante-room at Netley is an excellent portrait presented by his numerous friends, admirers, and pupils.

May his memory long continue to influence coming generations of medical officers in the School he loved so well!

J. F.

THOMAS HAWKSLEY, civil engineer, was born at Nottingham in 1807. He was educated as an architect and surveyor, but, having an inclination for mechanical pursuits, he studied diligently the sciences necessary to enable him to practise as a civil engineer, and with such success that in 1830 he undertook the construction of new waterworks for his native town. The knowledge and skill he exhibited in these works led to more practice in other districts, and in 1852 he removed his offices to London, where, before long, he took the highest rank in that branch of engineering having to do with water and gas supply, and with drainage and hydraulic works generally.

Mr. Hawksley was accustomed to say that he had constructed above 150 waterworks, many of the largest character; and that there were no important towns in Great Britain, and indeed very few great cities in the civilised world, in regard to which he had not been professionally consulted in some way or other. He is especially celebrated for having been the first to suggest and to carry into practice the system of "constant service" in water supply, which combined the most free and ample provision of water with the almost perfect

repression of waste, and with greatly improved sanitary conditions. The introduction of the system involved many difficulties and much opposition; but he always spoke of his success in it with great satisfaction and pride.

It must not be supposed that municipal engineering in the days of Mr. Hawksley's early practice meant simply building and mechanical operations. It involved often grave and novel considerations, and it was his merit to bring to bear upon them accurate scientific knowledge and careful study. The lucid and skilful manner in which he was in the habit of applying scientific principles to his professional practice was well known to engineers generally: "Mr. Hawksley's formulæ," "Mr. Hawksley's data," "Mr. Hawksley's general results," and so on, were continually adopted as familiar guides by his professional brethren, and were quoted as authorities against which there could be no appeal.

On one occasion he had to advise on the drainage of one of the largest towns in the kingdom, and a question arose involving some artificial hydraulic conditions of much greater magnitude than usual. Doubts were expressed as to the feasibility of his scheme, but Mr. Hawksley had a strong impression that the ordinary rules, based on comparatively small experiments, did not apply. He accordingly examined the question thoroughly, bringing to his aid certain recent hydraulic researches by eminent French mathematicians; and, with the help of the writer of this notice, he succeeded in showing the practicability of the plan by an amount of scientific evidence which, while it was perfectly new, was absolutely incontrovertible.

Mr. Hawksley was President of the Institution of Civil Engineers for the years 1872 and 1873, and of the Institution of Mechanical Engineers in 1876 and 1877.

In 1876 he was elected President of the National Association of Social Science, holding their meeting at Liverpool, when he gave an address especially remarkable for its happy application of statistics to sanitation. He was a clever and lucid writer, and his keen appreciation of scientific reasoning gave great weight to his opinions.

He was elected a Fellow of the Royal Society on the 6th of June, 1878, as being "especially distinguished for the application of Science to Hydraulic Engineering."

Mr. Hawksley was blessed with a constitution which prolonged his life and energy much beyond the ordinary lot of man. In the beginning of September, 1893, sixty-three years after his appointment as engineer to the Nottingham Waterworks, he undertook one of his customary tours of inspection of his works in progress in distant parts of England; but a fortnight afterwards he was attacked by a sudden and formidable disease, which his aged frame was not able to resist, and he died at his residence at Kensington on the 23rd.

W. P

John Tyndall was born at Leighlin Bridge, near Carlow, in Ireland, on the 21st of August, 1820, and the early years of his life, up to the age of 19, were spent in this village, where he received instruction in the school of one John Conwill, who seems to have been a man of somewhat original and independent character. Not much is known of the subjects taught to Tyndall in this school, but he certainly acquired there a very thorough knowledge of elementary mathematics and geometry. Classics did not form part of the curriculum; for, at the age of 27 he was still unacquainted with the Latin language.

On leaving school at the age of 19, he obtained an appointment as assistant in the division of the Ordnance Survey at Carlow. About two years later, Tyndall came to England and attached himself to a Manchester firm of railway engineers, by whom he was employed in levelling, surveying, and making out plans and estimates in accordance with the rules of the Board of Trade. In this occupation he seems to have spent about five or six years of his life, and the experience thus gained during the so-called railway mania, doubtless, contributed in no small degree to his subsequent love of pursuits which necessitated accuracy of measurement and logical reasoning.

In the year 1847, he became acquainted with the late George Edmondson, who, at that time, was endeavouring to introduce into a large private boys' school at Preston instruction in the elements of experimental science. In the spring of that year Mr. Edmondson undertook, at the instigation of the Socialists who were under the leadership of Robert Owen, to transform their abortive establishment, "Harmony Hall," into a school and agricultural college, which received the name of "Queenwood College." Here, for the first time in an English school, experimental science was practically taught in the laboratory and in the field, and Tyndall, although at considerable pecuniary loss to himself, was easily persuaded to become a teacher of mathematics and surveying in this new establishment, being chiefly influenced, as he himself declared, by the opportunity afforded him of working in a chemical laboratory.

Tyndall remained at Queenwood College, where he had an enthusiastic class of pupils who were greatly attached to him, until the autumn of 1848, when he accompanied the writer to Marburg, there to continue his study of chemistry in the laboratory of Professor Bunsen. He did not, however, confine his attention to chemistry, but attended also the classes of the professors of mathematics and physics. Indeed, by far the larger portion of his time, during his first year at Marburg, was spent in mathematical work.

In the year 1850 he graduated in the Philosophical Faculty, taking mathematics for his principal subject in *vivâ voce* examination, and, for the two subsidiary subjects, chemistry and physics. Before

admission to examination at Marburg, it is essential to present to the Faculty a memoir on some original investigation made by the candidate. Tyndall's dissertation was entitled, "Die Schraubenflaeche mit Geneigter Erzeugungs-Linie und die Bedingungen des Gleichgewichts für solche Schrauben," which shows that, at that time, Tyndall's knowledge of mathematics was superior to his acquirements in chemistry and physics.

Physical Researches.—About this time there came to Marburg, as extraordinary professor, an enthusiastic young physicist, afterwards well known as Professor Knoblauch, who exercised a profound influence upon Tyndall, and who was probably the main cause of the latter devoting himself, for the future, chiefly to physical science. It was in conjunction with Knoblauch that Tyndall made his first physical investigation, the results of which were published in the year 1850 with the title "On the Deportment of Crystallised Bodies between the Poles of a Magnet."

During the next thirty-three years Tyndall published 135 papers, or at the average rate of rather more than 4 per annum. From Marburg he migrated to Berlin, where he worked for about a year in Magnus's laboratory, continuing his researches on diamagnetism and magne-crystallic action, finally returning to England about the end of the year 1851 or the beginning of 1852. He took up his quarters again at Queenwood College, not as a teacher, but as a guest, awaiting the advent of some suitable appointment. At this time there was no physical laboratory in England, and consequently no chair of experimental physics. There was, it is true, a professor of physics at Owens College, Manchester, but the chair was occupied by a Cambridge wrangler, who, though an able mathematician, probably never made an experiment in his life. Tyndall had to wait until 1853, having in the meantime been an unsuccessful candidate for a professorship at Toronto.

On February 11th, 1853, he delivered, at the Royal Institution, his first public lecture "On the Influence of Material Aggregation upon the Manifestation of Force." This lecture, although of such an abstruse character, took his audience—mostly popular as it was—by storm. It concluded with the following graceful tribute to Faraday:—"This evening's discourse is, in some measure, connected with this locality; and, thinking thus, am led to inquire wherein the true value of a scientific discovery consists? Not in its immediate results alone, but in the prospect which it opens to intellectual activity, in the hopes that it excites, in the vigour which it awakens. The discovery which led to the results brought before you to-night was of this character. That magnet (pointing to the large electro-magnet at the Royal Institution) was the physical birthplace of these results; and if they possess any value they are to be regarded as the returning

crumbs of that bread which, in 1846, was cast so liberally upon the waters. I rejoice, ladies and gentlemen, in the opportunity here afforded me of offering my tribute to the greatest worker of the age, and of laying some of the blossoms of that prolific tree which he planted at the feet of the great discoverer of diamagnetism."

This phenomenal success with such a critical audience at once established Tyndall's reputation as a clear and powerful expositor of experimental science; and, in the following July, he was unanimously elected Professor of Natural Philosophy in the Royal Institution. It was in the physical laboratory of this Institution that the whole of Tyndall's subsequent work was performed, in so far as it was not work involving the personal observation of natural phenomena in the Swiss Alps and elsewhere; for, although he occupied the chair of physics in the Government School of Mines for several years, no laboratory was provided for him or his pupils in that Institution.

The following is Tyndall's own account of his first lecture given in his "Faraday as a Discoverer":—"In December, 1851, after I had quitted Germany, Dr. Bence Jones went to the Prussian capital to see the celebrated experiments of Du Bois Reymond; and influenced, I suppose, by what he heard, he afterwards invited me to give a Friday evening discourse at the Royal Institution. I consented, not without fear and trembling, for the Royal Institution was to me a kind of dragon's den, where tact and strength would be necessary to save me from destruction. On February 11th, 1853, the discourse was given, and it ended happily. I allude to these things, that I may mention that, though my aim and object in that lecture was to subvert the notions both of Faraday and Plücker, and to establish, in opposition to their views, what I regarded as the truth, it was very far from producing in Faraday either enmity or anger. At the conclusion of the lecture he quitted his accustomed seat, crossed the theatre to the corner into which I had shrunk, shook me by the hand, and brought me back to the table."

To return to Tyndall's first physical paper, published as a joint investigation by Knoblauch and himself:—by employing a method proposed by Dove, they examined the optical properties of crystals and found that these optical qualities went hand in hand with their magnetic observations. For a long time, these experiments led to the discovery of no fact of importance; but at length, the observers met with various crystals whose deportment could not be brought under the laws of magne-crystallic action as announced by Plücker. They also discovered cases which led them to imagine that this magne-crystallic action was by no means independent, as alleged, of the magnetism or diamagnetism of the mass of the crystal. In short, the more they worked at the subject the more clearly was it revealed to them, that the deportment of crystals in the magnetic

field was due, not to a force previously unknown, but to a modification of the known forces of magnetism and diamagnetism by crystalline aggregation. They found, for instance, that whilst Iceland spar, which had been adduced by Plücker and experimented on by Faraday, was, according to the law of Plücker, axially repelled by a magnet, it was only necessary to substitute, in whole or in part, ferrous carbonate for calcic carbonate, thus changing the magnetic but not the optical character of the crystal, to cause the axis to be attracted. They proved that the deportment of magnetic crystals is exactly antithetical to that of diamagnetic crystals isomorphous with the magnetic crystals, and showed this to be a general law. In all cases, the line which in a diamagnetic crystal set equatorially, always set itself in an isomorphous crystal axially. It was, moreover, shown that by mechanical compression other bodies were also made to imitate Iceland spar. The results of these experiments were published in the 'Philosophical Magazine' and in 'Poggendorff's Annalen,' and Tyndall subsequently, and apart from Knoblauch, continued these investigations in the laboratory of Magnus. The results and the conclusions drawn from them which form the first section of Tyndall's experimental researches, have never been called in question.

The great work of Tyndall's life, however, was not performed in the domain of magnetism, but in that of heat. Already, in the year 1852, he was experimenting on the transmission of heat through organic structures, and in October of that year he sent his first paper to the Royal Society, entitled "On Molecular Influences. Part I. Transmission of Heat through Organic Structures." As an illustration of the leisurely way in which such papers were treated in those days, this was not read until the 6th of January in the following year, and did not appear in the 'Transactions' until the year 1854, a year after it was read. The paper deals with the transmission of heat through wood, and the author expresses the laws of molecular action which he deduces from his experiments as follows:—1. At all the points not situate in the centre of the tree, wood possesses three unequal axes of calorific conduction, which are at right angles to each other. The first and principal axis is parallel to the fibre of the wood. The second and intermediate axis is perpendicular to the fibre and to the ligneous layers; while the third and least axis is perpendicular to the fibre and parallel to the layers. 2. Wood possesses three axes of cohesion which coincide with the axes of calorific conduction—the greatest with the greatest, and the least with the least. 3. Wood possesses three axes of fluid permeability which coincide with those of calorific conduction—the greatest with the greatest, and the least with the least.

These researches on the transmission of heat through organic structures were not afterwards continued, as Tyndall's attention was

soon after diverted from conduction to radiation; and his investigations on the action of gases and vapours upon radiant heat, continued for twelve years, constitute, by reason both of the experimental skill exhibited in their prosecution and the importance of the results obtained, the crowning achievement of his life. The first indication of the commencement of this work was given in a Friday evening lecture, delivered in the Royal Institution on the 10th June, 1859, "On the Transmission of Heat of different Qualities through Gases of different Kinds." At this lecture the apparatus used throughout his remaining investigations, with some modifications, was introduced to his audience. It consisted of a tube having its ends stopped air-tight by polished plates of rock-salt. This tube could be attached to an air-pump and exhausted, so that any required gas or vapour could be admitted into it. A thermo-electric pile being placed at one end of the tube and a source of heat at the other, the needle of an extremely sensitive galvanometer connected with the pile was deflected. After it had come to rest, the air was pumped from the tube and the needle was carefully observed, to see whether the removal of the air had any influence on the transmission of the heat. No such influence showed itself, the needle remaining perfectly steady. A similar result was obtained when hydrogen gas was used instead of air.

It now occurred to the experimenter to increase the sensitiveness of his apparatus by the use of a differential galvanometer. Under the influence of two sources of heat, one of which was caused to pass through the experimental tube, the astatic needle of the galvanometer was brought to zero by two powerful currents, which exactly neutralised each other. A few strokes of the air-pump were now sufficient to make the current from the thermo-pile at the end of the tube to predominate over its antagonist by 40° or 50°. On re-admitting the air the needle again fell to zero, thus proving beyond doubt that the air within the tube intercepted a portion of the radiant heat, the source of heat being one at a temperature at about 300° C. Instead of a differential galvanometer and two thermopiles, Tyndall afterwards used only one pile with the two sources of heat operating upon its opposite faces. The same method was applied to other gases, with most remarkable results, gases being found to differ amongst themselves with regard to their action on radiant heat as much as liquids and solids do. Some gases he found bearing the same relation to others that alum does to rock-salt. He found transparent and dry coal-gas to be exceedingly powerful in cutting off the radiant heat from a source at about 300° C., but when the lime light was placed at one end of the tube and the rays concentrated by a convex lens were sent through the tube, having previously been caused to pass through a thin layer of water, the coal-gas had no power to absorb the luminous heat thus transmitted through it. He drew from these

experiments the conclusion that planets, even at a great distance from the sun, might have atmospheres of such a character as to maintain upon their surfaces sufficient solar heat for the maintenance of life, such as we know it on the surface of our earth.

Tyndall's first paper communicated to the Royal Society on this subject was received on May 26, 1859. This was, however, only a preliminary note, and his first formal paper on the investigation formed the subject of the Bakerian Lecture delivered on the 7th February, 1861. In this lecture he enumerated the enormous difficulties he had to contend with in devising methods by which trustworthy results could be obtained. He relates how, for seven weeks, he worked from eight to ten hours daily, and had to abandon all the results as liable to certain errors. It was only after much labour and many failures that he constructed an apparatus which yielded consonant and trustworthy numbers. Shortly summarised, the results of this classical investigation may be thus stated:—1. Elementary gases scarcely absorb any perceptible amount of radiant heat. 2. All compound gases absorb proportions varying directly with the complexity of their molecules. Thus the vapour of ether was found to absorb, for equal volumes at maximum density, 100 times the quantity of radiant heat intercepted by the vapour of carbonic disulphide. The molecule of carbonic disulphide vapour contains only 3 atoms, whilst that of ether contains no less than 15. Nevertheless, the quality of the atoms constituting the molecule has also a profound influence upon the absorptive power. Thus, carbonic acid contains in its molecule the same number of atoms as carbonic disulphide, but at a tension of 1·2 in. its absorption is represented by the number 37, whilst the vapour of bisulphide of carbon at a tension of only 1 in. is represented by the number 62. Again, ethylic borate, which contains in its molecule no less than 25 atoms, has an absorptive coefficient, at only 0·1 in. tension, represented by the number 620.

Of all the molecules experimented upon, boric ethylate was the most complex and exercised the most powerful absorptive effect upon radiant heat from a source at 100° C. Whilst elementary and difficultly liquifiable gases exercise, as already stated, a scarcely sensible absorptive effect, easily liquifiable elementary gases and vapours, like those of chlorine and bromine, exert a very sensible action. Thus, whilst oxygen, nitrogen, and hydrogen are represented by the absorption coefficient of unity, chlorine is represented by the number 60, and bromine vapour, at the same tension, by 160. Compound molecules, though no more complex than elementary ones, exert a much more powerful absorptive action; thus bromine and hydrobromic acid both contain only 2 atoms in their molecules, nevertheless the absorptive coefficient of hydrobromic acid is more than six times as great as that of bromine.

In this paper, Tyndall also studied the radiation of heat by gases, and found that oxygen, hydrogen, and nitrogen are practically incapable of radiating heat from a source of comparatively low temperature, whilst the radiating power of four compound gases is expressed by the following numbers :—

Carbonic oxide	12
Carbonic anhydride	18
Nitrous oxide	29
Olefiant gas..............	53

Their radiative powers follow precisely the same order as their powers of absorption. He then proceeds to discuss the theoretical bearings of his experimental results. He draws attention to the enormous difference in behaviour towards radiant heat exhibited by mechanical mixtures of gases as compared with the same gases chemically combined. Thus hydrogen and nitrogen when mixed together in the proportion of 1 vol. of nitrogen to 3 vols. of hydrogen, produce a scarcely perceptible absorptive effect; whilst, when chemically united in the form of ammonia, they produce an enormous effect. Again, oxygen and hydrogen which, when mixed in their electrolytic proportion, show scarcely sensible action, when chemically combined in the form of aqueous vapour, exert a powerful action. So also with oxygen and nitrogen, which, when mixed, as in our atmosphere, both absorb and radiate feebly, when united as nitrous oxide, have their powers vastly augmented. Atmospheric air, freed from moisture and carbonic anhydride, and at a tension of 5 inches, did not effect an absorption equivalent to more than one-fifth of a degree of the differential galvanometer; whilst nitrous oxide of the same tension effected an absorption equivalent to 51°. Hence the absorption by nitrous oxide at this tension is about 250 times that of air. In like manner the absorption by carbonic oxide of this tension is nearly 100 times that of oxygen alone; the absorption of carbonic anhydride being about 150 times that of oxygen; whilst the absorption by olefiant gas of this tension is 1000 times that of its constituent hydrogen. But even the enormous action last mentioned was surpassed by the vapour of many volatile liquids possessing greater atomic complexity.

Tyndall visualised to himself the cause of this enormous difference. He considered that the compound molecules present broad sides to the ether, while the simple or elementary molecules do not; but, in consequence of these differences, the ether must swell into billows when the former are moved, while it merely trembles into ripples when the latter are agitated. In the interception of motion also, the former, other things being equal, must be far more influential than the latter.

Now, besides presenting broader sides to the ether, the association of atoms to form groups must, as a general rule, render their motion

through the ether more sluggish, and tend to bring the periods of oscillation into isochronism with the slow undulations of obscure heat, thus enabling the molecules to absorb more effectually such rays as have been made use of in his experiments. He concluded, however, that an agreement in period alone is not sufficient to cause absorption and radiation; but, in addition to this, the molecules must be so constituted as to furnish *points d'appui* to the ether. He remarks that the heat of contact is accepted with extreme freedom by rock salt, but a plate of this substance once heated requires a great length of time to cool. This effect is explained by the experiments of Balfour Stewart, which prove that the radiative power of heated rock-salt is extremely feeble. Periodicity, Tyndall remarks, can have no influence here, for the ether is capable of accepting and transmitting impulses of all periods, and the fact that rock-salt requires more time to cool than alum simply proves that the molecules of the former glide through the ether with comparatively smaller resistance, and thus continue moving for a longer time; while those of the latter, being extremely complex in comparison with rock-salt, present broad sides to the ether and speedily communicate to it the motion which manifests itself as heat. This power of gliding through still ether, possessed by the rock-salt molecules must, of course, enable the moving ether to glide round them, and no coincidence of period could, he thought, make such a body a powerful absorber.

Tyndall extended these experiments to the effect of odours on the absorption of radiant heat. He experimented upon the perfumes arising from patchouli, sandal wood, geranium, oil of cloves, otto of roses, bergamot, lavender, lemon, nearoli, portugal, thyme, rosemary, oil of laurel, cassia, camomile flowers, spikenard, aniseed, and, lastly, musk. Calling the absorptive power of the mixed nitrogen and oxygen of atmospheric air unity, the smallest absorption, namely, that of patchouli, was found to be 30; whilst the odour of cassia was 109, and that of aniseed 372. The most surprising result, however, was obtained with musk. It is well known that this substance goes on emitting its odour for months or even years without any perceptible loss of weight; yet, when it was placed in a small glass tube, and dry air was passed over it in the experimental tube, the inconceivably small amount of this odour gave an absorption expressed in one experiment by 74 and in a second by 72. Several kinds of tea treated in the same manner produced absorptions which varied between 20 and 28.

Ozone was also submitted to the same test, and was found to have an enormously greater absorptive power than ordinary oxygen. Electrolytic oxygen, which could only contain a small percentage of ozone, was found to have 126 times the absorptive power of ordinary oxygen. This result is extremely surprising in view of the univer-

sally received constitution of the molecules of oxygen and ozone, the molecule of ordinary oxygen containing 2 atoms, whilst the molecule of ozone contains only three.

Whilst Tyndall was pursuing these researches in London, Magnus was engaged in conducting a similar investigation in Berlin. The general agreement between the results of these two able experimenters was, as might be expected, very close. In one important respect, however, there was a striking divergence. This was in regard to the action of aqueous vapour upon radiant heat, Magnus having found that aqueous vapour had little or no action, whilst Tyndall found it to be a very powerful absorbent of the heat rays of low refrangibility. A long controversy ensued, each experimenter appeared to have full confidence in his own results, and the opinions of others were consequently far from unanimous until a paper by Tyndall, published in the 'Proceedings of the Royal Society' in 1881, and entitled "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter," finally decided the point, and proved in a startling manner that Tyndall was right. In this paper he describes his repetition of the ingenious experiments of Mr. Graham Bell, wherein musical sounds were obtained through the action of an intermittent beam of light on solid bodies. Entertaining the opinion that these singular sounds were caused by changes of temperature producing corresponding changes of shape and volume in the bodies impinged upon by the beam, Tyndall argued that if this be the case, and if gases and vapours really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids; and it seemed to him plain, moreover, that by this new method many of his previous results might be brought to an independent test. Highly diathermanous bodies, he reasoned, would produce faint sounds, while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, the measure of the absorption. The source of the intermittent beam was a Siemens lamp connected with a dynamo machine in the front of which was placed a rotating disk. The result shall be related in Tyndall's own words.

"Sulphuric ether, formic ether, and acetic ether, being placed in bulbous flasks, their vapours were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disk so that the intermittent beam passed through the vapour, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapours which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be least absorbent, the latter standing near the head of diathermanous vapours. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the

chloroform. With regard to the vapours of aniline, iodide of ethyl, iodide of methyl, and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat." The dry elementary gases, hydrogen, nitrogen, and oxygen, produced a musical note so feeble as to be heard only with attention; but when these dry gases were displaced by carbonic anhydride, the sound was far louder than that obtained from any of the elementary gases. Ammonia gas produced a loud musical note. Now came the crucial test to be applied to aqueous vapour.

Obviously, if dry air and moist air produced practically the same slight effect in the intermittent beam, the conclusion of Magnus would be right, whilst if the moist air produced a much louder sound, the correctness of Tyndall's result would be clearly demonstrated. He says: "In this relation the vapour of water was that which interested me most, and as I could not hope that at ordinary temperature it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost to its boiling point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapour. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but, notwithstanding the tenuity of the vapour, the sound extracted from it was not only distinct but loud. Three empty flasks, filled with ordinary air, were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced." Thus was this controversy finally set at rest.

Interpolated between the magnetic and thermal investigations, or carried on simultaneously with them, were his researches on the physical properties of ice. These investigations were carried out partly in the laboratory of the Royal Institution, but chiefly during vacation rambles in Switzerland. They involved a vast amount of laborious observation and acute reasoning, but in the light of recent experiments, it would be rash to predict that the theory of fracture and regelation, founded by Tyndall upon Faraday's original experiments, will maintain its place as the true and only explanation of the motion of glaciers.

In connection with his glacier work stand Tyndall's frequent ascents of Mont Blanc and other Swiss mountains. In one of these, namely the ascent of Mont Blanc in August, 1859, the writer accompanied him. The expedition was undertaken by Tyndall with the especial object of establishing several self-registering thermometric stations between Chamounix and the summit of the mountain.

Unfortunately the whole of these stations were swept away by avalanches during the following winter, except the one on the summit, which was seen to be intact next summer by the only mountaineer who made the ascent during that extremely unfavourable season. Probably this gentleman was too much occupied in admiring the grandeur of the surrounding scene to think of the importance of an observation of the minimum winter temperature at this great elevation. At all events he neglected to read off the self-registering instruments, and thus the expedition, so far as thermometric observations are concerned, was abortive. Nevertheless, it was not in other respects altogether unrewarded, as certain remarkable physiological and physical effects were observed during our stay of twenty-two hours on the summit of the mountain.

Almost immediately after arriving there, Tyndall became ill; he complained of headache, a burning sensation in the brain, and general lassitude, and expressed his belief that he was about to be seriously unwell. Scarcely half an hour had elapsed before all the guides and porters, nine in number, who had remained at the summit, complained loudly of headache, and expressed a strong desire to lie down and rest. Accordingly the erection of our tent was at once proceeded with. About 12 ft. below the summit of the ridge, and on its south side, a circular level plateau, about 10 ft. in diameter, was excavated in the snow, so as to form a level floor for the tent, the setting up of which did not occupy more than half an hour. As soon as it was ready, we were all glad to creep into it, the sun having become shrouded with fleecy clouds, which at once transformed the hitherto pleasing temperature into the piercing cold of a severe winter. The north wind had also increased in force, filling the air with clouds of snow blown from the terminal ridge of the mountain, and rendering exposure outside the tent by no means pleasant.

The cold blast of the north wind was not the only reason why the interior of our tent was so welcome, the general lassitude that had seized upon us all rendering lying down, even upon a bed of hard snow, a matter of urgent necessity. The indisposition of the whole party continued to increase; especially was this the case with Tyndall, who complained of fever, excessive thirst, and intense pain in the head. His pulse kept up to 100, but this was less alarming than the other symptoms, since the writer's own kept steadily at 120, although he was comparatively well during the first four hours of his sojourn upon the summit. Then a general lassitude stole over him also accompanied by headache.

Tea was the only liquid which was acceptable to us during the whole time of our stay on the summit. We had wine and brandy in abundance, but no one desired them; even champagne had a nauseous taste, and was far less acceptable than tea. For solid food there was

no desire; the writer ate only 2 oz. of bread on the summit of the mountain, and that quite in opposition to the will of the stomach. This distaste for food was common to the whole party. During the night, a thermometer inside the tent in which eleven people were closely packed, never sank below $-1°$ C., although the temperature outside was as low as $-17°$ C. The indisposition of the whole party continued, but there was little or no vomiting; and the prominent symptoms of mountain sickness seemed to be headache, with excessive lassitude and unwillingness to use the slightest physical, or even mental, exertion. The pulse was rapid but without any fever, except in the case of Tyndall, who continued alarmingly ill during the night. Snow wrapped up in a cloth and applied to his forehead and temples gave him some relief, but his thirst was insatiable; and as ice would scarcely melt in the warmest part of the tent, it was rarely that anything but snow could be obtained for him. Most of the party slept four or five hours, but both of us remarked that the peculiarity of our position developed a species of selfishness amongst the men, like that sometimes observed in cases of shipwreck, and which manifested itself in symptoms of insubordination and general discontent. Before leaving the horizontal position in the morning, or making any exertion, the writer's pulse was found to be still steady at 120, though unaccompanied by any feeling of feverishness. There was nothing unusual in respiration, and no difficulty of breathing. In short, lying there on the floor of the tent, there was nothing in our sensations by which we could have known that the tent was not pitched at the level of the sea on a frosty morning; there was no sensation which rendered the great rarefaction of the air perceptible.

Tyndall, who was now rapidly recovering, superintended the erection and furnishing of the thermometer post, and afterwards experimented on the thermal effect of the sun's rays; whilst the writer occupied himself with the collection of samples of air for analysis, and with experiments in the tent on the rate of combustion of stearin candles, which he had undertaken, at Tyndall's request, in order to test the correctness of the following statement made by Le Conte in Silliman's Journal of Science and Art. "Thus, a variety of well established facts concur in fortifying the conclusions to which we are led by *à priori* reasoning, namely, that the process of combustion is retarded by the diminution of the density of the air, whilst it is accelerated by its condensation." A comparison of the results obtained by burning these six candles for one hour at Chamounix, and for the same time on the summit of the mountain, completely refuted this statement; the amount of stearin consumed under the two widely different barometric pressures, was practically the same.

Another, and entirely unexpected, phenomenon, however, revealed itself to the writer in the course of these experiments. The candles

burning in the subdued light of the tent obviously gave a comparatively small amount of light; the lower and blue portion of the flame, which under ordinary circumstances scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of an eighth of an inch above the cotton, thus greatly reducing the volume of the luminous portion of the flame. These experiments were repeated, on returning to England, in artificially rarefied atmospheres, and led to the discovery of the law that the diminution in illuminating power is directly proportional to the diminution of atmospheric pressure.

With the exception of one or two of the porters, all the party felt a marked diminution in the symptoms of mountain sickness after 8 A.M. The rate of pulsation regularly decreased during the descent, notwithstanding the violent muscular exertion. After remaining steadily at 120 during the 22 hours spent on the summit, that of the writer dropped to 100 in the corridor, 80 on the Grand Plateau, and to 56 at Chamounix, his normal pulse-rate being 60.

Biological Researches.—About the year 1875, Tyndall became interested in the question of spontaneous generation, which at that time was exciting a considerable amount of attention, especially in the medical profession. Pasteur had pronounced spontaneous generation a chimera, and expressed his undoubting conviction that, this being so, it is possible to banish zymotic diseases from the earth. To the medical profession therefore, and through them to humanity at large, this question was one of the last importance. But the state of medical opinion about it at that time was extremely conflicting and unsatisfactory. With a view to the possible diminution or removal of this uncertainty, Tyndall determined to apply the exact methods of experimental physics to this difficult biological problem. He had a number of chambers or cases constructed, each with a glass front, its top, bottom, back, and sides being of wood. These chambers were so contrived that infusions of various kinds could be exposed to germless air after being boiled. He thus had these infusions exposed to an atmosphere of oxygen, nitrogen, carbonic anhydride, ammonia, aqueous vapour, and all the other gaseous matters which mingle more or less with the air of a great city. He had them moreover "untortured" by calcination, and unchanged even by filtration or manipulation of any kind, for the air was rendered germless by subsidence. The question which he set himself to answer was this :—"Can air thus retaining all its gaseous mixtures but self-cleaned from mechanically suspended matter, produce putrefaction?" To this question both the animal and vegetable worlds gave him a decided negative. Among vegetables, experiments were made with hay, turnips, tea, coffee, and hops, and were repeated in various ways with both acid and alkaline infusions. Among animal substances he experimented with beef,

mutton, hare, rabbit, kidney, liver, fowl, pheasant, grouse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, and oysters. The result was that infusions of these substances, exposed to temperatures varying from 27° C. to 32° C. in these germless atmospheres, in no single instance underwent putrefaction or developed the slightest amount of bacterial life. On the other hand, infusions of the same substances, exposed to the common air of the Royal Institution laboratory, all fell into putrefaction in the course of from 2 to 4 days; no matter where the infusions were placed, they infallibly became offensive in the end. The number of tubes containing infusions was multiplied until it reached 600, but not one of them escaped infection.

To detect the floating germs in the air, Tyndall employed a powerful beam of light from which the eye of the observer was carefully screened. "When the track of a parallel beam in dusty air," says Tyndall, "is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finest portions of the suspended matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that the matter potential as regards the development of bacterial life is to be sought." He was thus employing for the detection of suspended matter in air an instrument far more delicate than the microscope, and he reasons upon the results of his experiments as follows:—"But though they are beyond the reach of the microscope, the existence of these particles, foreign to the atmosphere, but floating in it, is as certain as if they could be felt between the fingers, or seen by the naked eye. Supposing them to augment in magnitude until they come, not only within the range of the microscope, but within range of the unaided senses; let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand, filled with nutritious earth, with which we mix our unknown particles, and that, in forty-eight hours subsequently, buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment, when repeated over and over again, to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust or mineral particles, or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly consider the experiment in the flower-pot as clearing up our pre-existing ignorance. We should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the

seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been spontaneously generated. This reasoning applies, word for word, to the development of bacteria from that floating matter which the electric beam reveals in the air, and in the absence of which no bacterial life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of bacterial life developed from dead dust can ever gain currency among the members of a great scientific profession."

During the course of these experiments, Tyndall made the very important discovery of the necessity for the intermittent application of heat for the attainment of absolute sterility. He found that the spores of certain bacteria could resist a boiling temperature for five hours, although the fully-developed bacteria are killed by the application of the same temperature for a few minutes. Hence the now universal practice amongst bacteriologists of heating their infusions to the requisite temperature, for a few minutes on three consecutive days. The first heating destroys all the fully-developed organisms; before the second takes place, the remainder will, in all probability, have developed and be likewise destroyed; but certainly, by the heating on the third day, not a single germ will escape destruction.

It is not possible, within the space of an obituary notice, to do more than give a mere outline of the enormous amount of work accomplished during the thirty-three years of Tyndall's active life. Nothing has been said here of his most interesting work on acoustics, and many memoirs on isolated subjects have been entirely ignored.

As his colleague for six years in the Royal Institution, the writer had ample opportunity of judging of Tyndall's remarkable experimental skill and untiring perseverance in the search after truth. So long as any result was doubtful, no amount of labour was considered too great to eliminate all uncertainty. His cleverness in devising new forms of experiment for the interrogation of nature, was most striking; and he never allowed himself to trust hypothesis where appeal to experiment was possible.

In the year 1886, Dr. Tyndall's health entirely broke down, mainly through overwork, and the managers of the Royal Institution granted him a year's holiday; but, although this relief was of some benefit to his health, at the end of the year he felt compelled to resign his appointment. In accepting his resignation the managers, in their meeting in April, 1887, recorded the following resolution:—"The managers desire to record the expression of their deep regret that the state of Dr. Tyndall's health should have rendered necessary the resignation of his position of Professor of Natural Philosophy at the Royal Institution, and that it should have compelled the managers to accept that resignation. They also desire that there should be recorded

the expression of their thorough appreciation of the unremitting and most valuable services which, during the long period of 34 years, Dr. Tyndall has rendered to the Royal Institution in carrying out the duties of his office—services which not only have upheld and have advanced the position of the Royal Institution, but have benefited science and the world at large."

Professor Tyndall, on his withdrawal from the Institution, declined to receive any pension or pecuniary testimonial in recognition of his services; and, in severing his long connection with it, desired only to carry with him the friendly recollection and goodwill of the members. At the same meeting of managers, it was resolved unanimously that Dr. Tyndall be nominated for election at the next general monthly meeting, on Monday, May 9th, as Honorary Professor of Natural Philosophy; and he was so elected on that day. The managers also instituted, at the same time, an annual course of lectures to be called the Tyndall Lectures.

Dr. Tyndall held the post of Examiner in the Royal Military Colleges and in the University of London. In 1866, he succeeded Faraday as scientific adviser to the Trinity House, and occupied this position for 17 years. In 1872 he was invited to lecture in the United States, and realised a considerable sum of money from the large audiences attracted by his eloquence and experimental skill. The whole of this sum, amounting to between £6,000 and £7,000, he generously devoted to the encouragement of scientific training in the United States, dividing it equally between Columbia College in New York, Harvard College Boston, and the University of Pennsylvania at Philadelphia.

In 1876, he married Louisa, eldest daughter of the late Lord Claud Hamilton, and received, on the occasion of his marriage, a purse of 300 guineas from the members of the Royal Institution, and a medallion bust of himself, in marble, from his fellow members of the X—— Club.

Dr. Tyndall was elected a Fellow of the Royal Society in 1852. In 1853 a Royal Medal was awarded to him for his researches on magnecrystallic action. He also received the Rumford Medal, in 1864, for his researches on the absorption and radiation of heat by gases and vapours. He was D.C.L. (Oxon.), LL.D. (Cantab., Dubl., et Edin.), and an honorary member of a large number of learned societies at home and abroad.

The last years of his life, after his retirement from the Royal Institution, were clouded by repeated attacks of illness. In the autumn of 1893, his usual sojourn on the Bel Alp appeared to effect a substantial improvement in his health; but, almost immediately on his return, he had a serious relapse, from which he was gradually recovering, when, on the 4th of December, he died from the effects of an over-

dose of chloral accidentally administered to him in mistake for sulphate of magnesia. In the presence of a very large number of his friends and admirers, his remains were interred in Haslemere Churchyard on the 9th of December, the coffin bearing the following inscription:—"John Tyndall, died December 4th, 1893, aged 73 years."

E. F.

Sir Samuel White Baker was born at Thorngrove, near Worcester, on the 8th June, 1821. He was the eldest son of Samuel Baker, of Lypiatt Park, Gloucester.

At the age of twenty-four he went to Ceylon, where he was engaged in agricultural pursuits in company with one of his brothers.

He has given an interesting account of life and sport in Ceylon in two works, which he afterwards published.

After quitting Ceylon, where it is understood his farming operations had not been a success, he was engaged for some time in Eastern Europe on the Ruschuk and Varna Railway.

In 1860, Speke and Grant set out from Zanzibar, commissioned by the Royal Geographical Society of London to follow up the discovery of the Victoria Lake and trace the Nile should it be found to be connected with that Lake. Baker determined to go at his own expense by way of Cairo, in order to meet his friends, and, if possible, render them the help of which they would stand in need. Setting out in 1861, he ascended the Nile, and was fortunate in meeting Speke and Grant at Gondokoro, thus enabling them to complete their journey, while he, aided by indications given him by these travellers, pressed south and discovered the Albert Nyanza, through which the Nile was found to flow.

On Baker's return to Europe he received, in recognition of his services, the honour of knighthood. He was also awarded for his geographical discoveries, the Gold Medal of the Royal Geographical Society and that of Paris, and was elected an Honorary M.A. of Cambridge, where he afterwards was Rede Lecturer in 1874.

In the year 1870 he returned to Egypt in the service of the Khedive, he ascended the Nile to Gondokoro in command of a well-equipped Egyptian force, and was for two years engaged in establishing the claims of Egypt to dominions on the Upper Nile. For this he was made a Pasha and Major-General in the Turkish service.

The remainder of his life was spent in foreign travel and at his home at Sandford Orleigh, near Newton Abbot, where he died December 30th, 1893. He became a F.R.S. in 1869.

J. K.